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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Combining visible and infrared radiometry and lidar data to test ice clouds optical properties" *by* A. Bozzo et al.

Anonymous Referee #2

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General comments

This is an interesting paper that utilises visible and infrared measurements of cirrus to test the physical consistency of assumed optical properties. This is an important topic of research since it is critical to show that optical parameterizations used in climate or weather prediction models have value in simultaneously predicting visible and infrared radiances that are within measurement uncertainty. This is not only important in terms of predicting the net radiative effect of cirrus in climate models but also simulating visible and infrared radiances in both climate and weather prediction models and for this latter application the phase function is critically important.

However, for the assumed ice crystal habit distribution used in the paper the authors do not find that the single-scattering properties predicted by the shape distribution are

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Discussion Paper

physically consistent across the near-ir and infrared spectrum, if they used the retrieved optical depth to simulate the MAS measurements. However, they find that the opposite is true if they used the retrieved re and optical depth from MODIS to simulate those measurements. The paper addresses the question as to why this contradiction might arise. The authors conclude that it is to do with the assumed phase function used in the short-wave.

The major problems with the paper are that there is no truth (i.e., in situ measurements of the PSD) or sufficient simulation experiments to exclude a number of other possibilities that might cause the apparent contradiction. Although, the paper does demonstrate the difficulty of addressing the contradiction without adequate in situ data, so from this point of view it would be of interest to publish the paper, if the following concerns can be addressed.

Major concerns

1. Firstly, given the lidar vertical extinction profiles shown in Fig. 8, why should the authors believe they can demonstrate physical consistency across the spectrum when the cloud they are using is demonstrated to be vertically inhomogeneous? Only if the cloud was observed to be 'ideal' in the sense that the cloud was observed to be suitably homogeneous could a test of physical consistency be performed. Or have the authors used a particular portion of the cloud that appears to be homogeneous? If so, this should be more clearly shown.

2. The shape of the PSD is very important in determining the bulk single-scattering properties of the cloud. The definition used by the authors is inadequate with the parameters of the PSD poorly defined. The authors assume that mu=0 so the PSD is assumed exponential. The choice of lambda becomes critical as the lidar will become very sensitive to these small particles, after all the lidar measurement is critical to the paper. The choice of lambda must be stated together with sensitivity tests to determine how the choice of lambda affects their results. The authors should also state the range

ACPD

10, C1823–C1826, 2010

of D the paper considers. I am concerned that the authors consider a size of zero! Since ice crystal size in cirrus does not vanish, though this assumption results in an exponential PSD that does not compare to reality and their modelled extinction will be biased to very small particles. The authors should perform a series of truncations of the PSD at the small particle end to determine the size to which the lidar becomes sensitive, and then use that size as their minimum D.

3. A further sensitivity test the authors should include is the impact of vertical inhomogeneity on their results. It is suggested the authors assume a three-layer model with the PSD from 2 above varying in each of the three layers. This variation in vertical inhomogeneity could result in differences across the spectrum due to the differing depth of penetration of each wavelength.

Since no in situ information is available the above simulations are important to perform as the forward modelling in the case of Fig. 8 is not just about the optical properties but it is also about the PSD and the vertical structure of the cloud.

Other comments

1. Title: clouds -> cloud

2. Introduction page 7217. Ham et al. -> Han et al and throughout rest of paper.

3. line 12 7217 “input of a. . .” -> ‘input to a. . .’

4. line 15 page 7218 Were the cloud decks single layer ? with no cloud beneath and over the sea ? If so these conditions should be stated in this paragraph.

5. Line 29 7218 ‘..in last section..’ -> ‘in the last section’

6. Line 15 page 7222. What is the definition of effective particle size used in this paper ? The definition should be fully stated and is the definition the same as that used for the MODIS retrieval? Since re is used and De elsewhere in the paper, it is important to be consistent in this regard.

ACPD

10, C1823–C1826, 2010

7. Line 4 page 7221. The RT model is plane-parallel and assumed to be homogeneous? If so this should be stated.

8. It is stated on page 7225 line 1 that the channel centred on 1848 cm-1 did not work properly, this being the case why is it included in Figure 3 ? as this contradicts the main statements given in the text of the paper describing figure 3.

9. On page 7234 Table 5 is mentioned, do the authors mean Table 2?

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10, C1823–C1826, 2010